and, by oxidation, to force-production." The great question of the relation of nitrogenised and non-nitrogenised matter to external body-work performed is entered into in considerable detail, and the important experiments of Fick and Wislicinus, Parkes, and Austin Flint, are described in full ; to them being added others, performed by Mr. Mahomed in the author's laboratory, on the length of time required for the elimination of the products of metamorphosis of an increased amount of nitrogenised food, from which it may be inferred that urea is produced and eliminated within the three hours following the ingestion of the nitrogenised matter.

It is shown that the original theory of Liebig, in which it is assumed that muscular action involves the destruction of muscular tissue, which till lately has been so generally accepted, "although, in reality, constituting a speculative proposition, unsupported by anything of the nature of proof," is opposed to all the results of recent investigation, and that if it were true "we should have to look upon nitrogenous alimentary matter as forming, through the medium of muscular tissue, the source, and the only source, of muscular power. The renewal of muscular tissue for subsequent oxidation in its turn, and evolution of muscular force, would thus constitute one of the functions of nitrogenous alimentary matter ; and on its supply would accordingly depend our capacity for the performance of muscular work." Great stress is laid on the necessity for the combination of nitrogenised with non-nitrogenised food for the sustenance of the body in a vigorous condition; and Mr. Savory's experiments on this point are shown to be quite insufficient to prove the inference which has been frequently drawn from them, namely, that nitrogenous matter, combined only with the appropriate saline principles, suffices for the maintenance of life.

The author reduces the unnecessarily extensive literature on the action of alcohol, which is so very negative in character, into a very moderate space, remarking that "from a review of the evidence as it at present stands, it may reasonably be inferred that there is sufficient before us to justify the conclusion that the main portion of the alcohol ingested becomes destroyed within the system ; and if this be the case, it may be fairly assumed that the destruction is attended with oxidation and a corresponding liberation of force, unless, indeed, it should undergo metamorphosis into a principle to be temporarily retained, but nevertheless ultimately applied to force-production. The subject appears to me to be open to physiological as well as chemical investigation, and probably some additional light may be hereafter thrown upon it by an approach through the former channel."

The discussion of the sources of each of the different most important articles of diet is followed by a concise account of its practical value. In the present time of excessive tea-drinking, the following description of the action of tea is of particular interest. "To express in a few words the advantages derivable from the use of tea, it may be said that it forms an agreeable, refreshing, and wholesome beverage, and thereby constitutes a useful medium for the introduction of a portion of the fluid we require into the system. It secures that the water consumed is safe for drinking by the boiling which is necessitated as a preliminary operation in making tea. It cools the body when hot, probably by promoting the action of the skin ; and warms it when cold, by virtue, it would seem, of the warm liquid consumed. In a negative way it may prove beneficial to health by taking the place of a less wholesome liquid. Through the milk and sugar usually consumed with it in England, it affords the means of applying a certain amount, and not by any means an insignificant amount, viewed in its entirety, of alimentary matter to the system. Experience shows that it often affords comfort and relief to persons suffering from nervous headache. It also tends to allay the excitement from, and counteract the state induced by, the use of alcoholic stimulants ; and further, on account of its anti-soporific properties, like coffee, it is useful as an antidote in poisoning by opium."

Besides the important purely physiological problems that are entered into in the work before us, there are so many which have a strictly practical bearing, and they are treated in so clear and impressive a manner, that the ordinary reader cannot but feel that he has derived great benefit from a careful study of its contents. Much stress is laid in the chapter on Practical Dietetics on the importance of a midday meal :—"A fairly substantial meal should be taken at this time, and it does not signify whether it goes under the name of luncheon or dinner." Carnivorous animals apparently thrive best when fed at long intervals ; herbivorous, when they are constantly eating. Man being omnivorous, his food should be taken at intervals of much less duration than the carnivora, and therefore in diminished quantities at each, three fairly substantial meals during the day, at intervals of five or six hours being found the best in the long run. "There are many business or professional men who, after leaving home for their office or chambers in the morning, do not taste food, or, if they do, take only a minute quantity, until they return in the evening. Actively engaged all day, their system becomes exhausted, and they arrive home in a thoroughly jaded or worn-out condition. They expect that their dinner is to revive them. It may do so for a while, but it is only a question of time how long this system can be carried on before evil consequences arise." It is therefore stated as a *sine quâ non* that the interval between breakfast and late dinner should be broken by a repast about half-way between them.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Physical Axioms

MR. COLLIER's letter demands from me a reply, which I will endeavour to make as brief as possible.

Mr. Spencer, instead of answering the difficulties which I had shown his *à priori* view of the Second Law of Motion to involve, only noticed my remarks to dismiss them summarily with the lofty sentence that I "proposed to exemplify unconsciously-formed preconceptions," and had committed an absurd blunder in so doing. And now, because it did not appear to me worth while, at the expense of your space and your readers' patience, explicitly to repudiate any such lofty purpose, and so, adopting Mr. Spencer's words, I merely called attention to the fact that the example (of whatever it might be considered to be an exemplification) was of Mr. Spencer's own choosing, I am charged by Mr. Spencer's follower, Mr. Collier, with having "confused issues," which I neither raised nor accepted. If Mr. Collier will

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do me the favour of reading my original note again, he will find that the object of my remarks was simply to test the truth of a definite assertion by Mr. Spencer that "the Second Law of Motion is an immediate corollary of the preconception of the exact quantitative relation between cause and effect." It was entirely beside my purpose to discuss the general psychological question of the formation of conceptions or preconceptions farther than as it is involved in the truth or otherwise of this particular assertion. Mr. Collier's note is therefore, as far as regards my remarks, entirely irrelevant and needs no other reply than to invite him, as Mr. Spencer declines to do so, to answer the simple and definite questions proposed by me as difficulties which Mr. Spencer is bound to answer, unless he is prepared to admit that he was wrong in the assertion on which I commented.

I have assumed throughout that Mr. Spencer means to assert that the Second Law of Motion is *involved in*, not merely that it *involves*, a particular preconception. And yet this latter is all that Mr. Collier asserts in the summing up of Mr. Spencer's argument, with which he concludes his note. If Mr. Collier truly represents Mr. Spencer, I can only say that, while the assertion may be admitted to be true, it certainly appears to me to be so trite as to be hardly worth formulating. The whole question turns on the distinction between "involving" and "being involved in," which I suppose Mr. Spencer and Mr. Collier would regard as an important one, though it is difficult in some cases to make out distinctly from their language and their line of argument which they mean to imply.

Passing in conclusion beyond the particular issue to which I have hitherto confined myself, I would remark that to my mind all that Mr. Spencer's and Mr. Collier's illustrations prove is that, while unconscious experiences (whether individual or inherited) may give rise to certain general, but (except in the very simplest cases) vague, preconceptions, it is only when these preconceptions are wedded to consciously-made observation or experiment that they cease to be barren generalities and give birth to the fruitful laws of Physical Science. To a mathematician, at any rate, it is almost ridiculous to observe how little either Mr. Spencer or Mr. Collier seem to realise the great gap between the indefinite observation that two things always increase and decrease simultaneously, and the definite conclusion that they are proportional to one another. For example, it is hardly a parody of Mr. Collier's remarks to say:—"A child discovers that the greater the angle between his legs the greater the distance between his feet, an experience which implicates the notion of proportionality between the angle of a triangle and its opposite side;" a preconception, as it appears to me, with just as good a basis as that whose formation Mr. Collier illustrates, but one which, as I need hardly add, is soon corrected by a conscious study of geometry or by actual measurement.

Harrow, May 25 ROBT. B. HAYWARD

MR. COLLIER'S letter, NATURE, vol. x. p. 43, is even more astonishing than anything that Mr. Spencer has written. A mathematician who reads it feels something like Alice behind the looking-glass; and perhaps behind the looking-glass it may be "a question pertaining to the psychological basis of inductive logic," with which mathematicians, as such, have nothing to do. But in this world, this side the looking-glass, in which forces are measured and effects are measured, Mr. Collier's letter is very perplexing.

For example, after giving several instances in which a greater force produces a greater effect, Mr. Collier proceeds: "The experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced: and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved."

Does Mr. Collier know what proportionality means? Does any one of the experiments indicated prove that where effort is doubled the result is *doubled?* The child pulls his boat by a string through the water; if he pulls twice as hard does he pull it *twice* as fast?

It seems to me that the people on the other side of the looking-glass think perfect quantitative equivalence (however measured) means the same as proportionality; and are willing to raise first the general result of experience, that greater forces produce greater effects, into an axiom of exact quantitative equivalence (without troubling themselves to consider how quantity is to be estimated), and then to accept Newton's Second Law as an instance of this quantitative equivalence, without showing any connection between quantitative equivalence and direct proportionality in that instance or in any other.

A SENIOR WRANGLER

Ocean Circulation

MR. CROLL will doubtless be of opinion that as my "theories" show such an utter ignorance of "even the elements of physics and mechanics," I can employ my time much better in acquiring some knowledge of those sciences, than in continuing to discuss the subject with him.

I shall be glad to be allowed to state to the readers of NATURE, as I have to those of the *Philosophical Magazine* (May), other grounds on which I must decline to prolong this discussion.

1. Mr. Croll has charged me (Phil. Mag. for March, p. 177, note) with a serious misstatement in regard to the mean annual rate of the Gulf Stream, which he affirms to be *nearly double* what I have represented it. Now *my* statement was avowedly based on the *average of the whole year's observed rates;* whilst Mr. Croll has taken as the basis of *his* the arithmetical *mean* between the maximum and the minimum. It has been said in disparagement of statistics that "anything can be proved by figures;" and Mr. Croll, who is nothing if not a statistician, seems to me to justify the imputation, for the adoption of his method would make the *average* number of children of a marriage to be at least *ten!*

2. Mr. Croll, in asserting that I have left out of consideration "the fact that the sea is salter in intertropical than in polar regions, and that this circumstance, so far as it goes, must tend to neutralise the difference of temperature," has only exhibited his own ignorance of a very important fact of Ocean Physics—the *low* salinity of equatorial surface-water; which was ascertained in Kotzebue's voyage fifty years ago, has been confirmed by many later series of observations, has been repeatedly cited in text-books, and has been adduced by myself as an indication that polar water is continually ascending from the bottom to the surface under the equator. But further, not only has this fact been confirmed by the *Challenger* observations, but so remarkable an accordance has been shown by them to exist between the low specific gravity of equatorial *surface*-water and that of equatorial *bottom*-water, as strongly to indicate that, as the latter is certainly polar, the former is so also. It suited Mr. Croll's purpose, however, with these observations before him, completely to ignore them, and to state as fact what is the precise contrary of facts.

3. According to Mr. Croll and his anonymous authority, the Astronomer Royal must be unfamiliar with even "the elements of physics and mechanics;" for, speaking from the chair of the Royal Society in 1872, he explicitly expressed his acceptance of the doctrine I advocate, as "certain in theory and supported by observation." The eminent meteorologist, Prof. Mohn, of Christiania, also, who expressed to me in writing last year his acceptance of it, must be equally ill-informed; as, too, must be Dr. Meyer of Kiel, who has been engaged for four or five years past in the investigation of the physics of the Baltic, the North Sea, and their connecting channels, and who has satisfied himself so completely of the power of small differences of specific gravity to put large bodies of water in motion. I have *nowhere said* that no eminent physicist shares Mr. Croll's objections; though I have not myself met with such a one.

I regret to have been forced, by the personal attacks in which Mr. Croll has latterly thought fit to indulge, thus to retort upon him. Henceforth I shall not consider myself called upon to take any notice of assertions and arguments which I do not find to exert the least influence on the opinions of the eminent scientific men with whom it is my privilege to associate.

WILLIAM B. CARPENTER

Glacial Period

IN answering Mr. Bonney's letter in NATURE, vol. x. p. 44, I shall confine myself to the consideration of his second objection to my theory, as the precise southern limit of the glacial action is not of present importance, and the height of the Scandinavian sea-beaches is irrelevant to the inquiry.

Mr. Tiddemann, in an admirable paper On the glaciation of North Lancashire (Quart. Journ. Geol. Soc., vol. xxviii. p. 471), has mapped out the course of the ice as shown by scratched rocks, lines of transported boulders, carriage southwards of local

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